44th Robert H. Goddard Memorial Symposium
Greenbelt, Maryland
March 15, 2006
Keynote Address

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It is a privilege for me to speak in this Symposium. My first job as a scientist, before I went on to graduate school, was at Goddard Space Flight Center. I had worked there during the summer of 1961, and returned as a full time employee in what was then called the Thermal Systems branch in the summer of 1962. Goddard was booming in those days, and the challenge of making scientific instruments work in the space environment attracted many fine scientists and engineers. I worked with a team trying to understand and optimize the properties of materials that could be used as thermoelectric generators for space applications, which shows you how broadly the spectrum of science and technology must extend to support missions in space. In the fall of 1963 I became a NASA graduate trainee in Stanford's then-new Department of Applied Physics, and ever since have combined my love of basic science with an interest in practical applications. The topic of this year's Symposium, *"… Engineers, Scientists and the Vision"* reflects the combination of mental attitudes needed to accomplish great things in space, and I am pleased to add a few thoughts of my own this morning on these topics.

I am always puzzled by debates over the vision for space exploration because the choices are so constrained by physical reality. We humans dwell in a vast universe whose chief features only became apparent during the twentieth century. We have known for a long time that a huge gap separates the objects trapped by the gravity of our star, the Sun, and everything else. Information about phenomena beyond that gap can come to us only through the rain of photons and other elementary particles spewed out by the awesome processes of the cosmos. Our observations of that part of space began in prehistoric times and they continue to sustain the growth of science in our era. Phenomena on our side of the interstellar gap, in what we call the Solar System, are potentially amenable to direct investigation and manipulation through physical contact, and can reasonably be described as falling within humanity's economic sphere of influence. As I see it, questions about the vision boil down to whether we want to incorporate the Solar System in our economic sphere, or not. Our national policy, declared by President Bush and endorsed by Congress last December in the NASA authorization act, affirms that, *"The fundamental goal of this vision is to advance U.S. scientific, security, and economic interests through a robust space exploration program."* So at least for now the question has been decided in the affirmative.

The wording of this policy phrase is significant. It subordinates space exploration to the primary goals of scientific, security, and economic interests. Stated this way, the "fundamental goal" identifies the benefits against which the costs of exploration can be weighed. This is extremely important for policy making because science, security, and economic dimensions are shared by other federally funded activities. By linking costs to these common benefits it becomes possible, at least in principle, to weigh investments in space exploration against competing opportunities to achieve benefits of the same type.

I want to stress how very different this kind of thinking is from the arguments that motivated America's first great space vision, the Apollo program. President Kennedy launched the Apollo program during an intense period in the Cold War, four years after the Soviets launched the first Sputnik satellite. In his 1961 message to Congress, Kennedy said of sending a man to the moon and returning him safely that, *"No single space project in this period will be more impressive to mankind, or more important in the long-range exploration of space; and none will be so difficult or expensive to accomplish..."* The tone of this message clearly conveys the intention to send a signal to the world that America *will* lead the way into space, and this spirit remains a vital factor in our ability to accomplish great feats of engineering to get us there. The Apollo program was what mathematicians call an "existence proof," a demonstration that a problem does have a solution and that efforts to discover its details will not be in vain. Like all firsts, it was unique. No subsequent space endeavor can be quite like it. President Bush's vision also declares the will to lead in space, but it renders the ultimate goal more explicit. And that goal is even grander. The ultimate goal is not to impress others, or merely to explore our planetary system, but to *use* accessible space for the benefit of humankind. It is a goal that is not confined to a decade or a century. Nor is it confined to a single nearby destination, or to a fleeting dash to plant a flag. The idea is to begin preparing now for a future in which the material trapped in the Sun's vicinity is available for incorporation into our way of life.

Given the expense of climbing out of Earth's gravity well, the natural course of space development begins with objects trapped in Earth orbit, including the Moon, followed by objects trapped in solar orbits near the Earth's, and then extending opportunistically to other destinations. The first stage of exploiting cislunar space is already well advanced, partly because applications have been found that can be achieved with small payloads and yet whose value to society exceeds the cost of launch. It is likely that these near-Earth applications will always dominate the use of space because Earth is where the people are, as well as the environment that sustains them. We must never forget that within our Solar System the object most important for humankind is Earth, and Earth-oriented space applications merit priority in a balanced portfolio of public investment.

The Moon has unique significance for all space applications for a reason that to my amazement is hardly ever discussed in popular accounts of space policy. The Moon is the closest source of material that lies far up Earth's gravity well. Anything that can be made from Lunar material at costs comparable to Earth manufacture has an enormous overall cost advantage compared with objects lifted from Earth's surface. The greatest value of the Moon lies neither in science nor in exploration, but in its material. And I am not talking about mining helium-3 as fusion reactor fuel. I doubt that will ever be economically feasible. I am talking about the possibility of extracting elements and minerals that can be processed into fuel or massive components of space apparatus. The production of oxygen in particular, the major component (by mass) of chemical rocket fuel, is potentially an important Lunar industry.

What are the preconditions for such an industry? That, it seems to me, must be a primary consideration of the long range planning for the Lunar agenda. Science studies provide the foundation for a materials production roadmap. Clever ideas have been advanced for the phased construction of electrical power sources – perhaps using solar cells manufactured in situ from Lunar soil. A not unreasonable scenario is a phase of highly subsidized capital construction followed by market-driven industrial activity to provide Lunar products such as oxygen refueling services for commercially valuable Earth-orbiting apparatus. This is consistent with the space policy statement that the U.S. will *"Develop the innovative technologies, knowledge, and infrastructures both to explore and to support decisions about the destinations for human exploration".*

I watched the live video coverage of Neil Armstrong taking the first footsteps on the Moon, and I was tremendously excited by it. To actually do something productive on the Moon would validate and justify the risk and expense of those early ventures and create an entirely new level of excitement. The operations I have described are intricate but many could be accomplished robotically. It is difficult for me to imagine, however, that such a complex activity could be sustained without human supervision and maintenance. This, in my view, is the primary reason for developing the capacity for human spaceflight to the Moon. It is a pragmatic reason and more likely to be sustainable over the decades necessary for success than curiosity or even national prestige.

Where does Mars fit into this picture? At the present time, much commentary to the contrary, we do not know how to send humans to Mars and return them safely within a reasonable cost envelope. The whole point of the vision, however, is to make the Solar System accessible, and Mars and the asteroids whose orbits penetrate Mars orbit are the nearest objects suitable for development beyond the Moon (I am excepting Venus for its high surface temperature). The current vision policy document says the U.S. will *"Extend human presence across the solar system, starting with a human return to the Moon by the year 2020, in preparation for human exploration of Mars and other destinations."* It does not propose a date for a human Mars mission. The cost and safety of a human Mars mission are very scenario-dependent, and I hesitate to say anything more about it. There is no question, however, that the expense of such a mission would be vastly reduced if the bulk of its fuel and massive components could be obtained from materials, and manufactured, outside Earth orbit. The Moon is a logical place to do this. As to the motivation for a human expedition to Mars, there is an obvious prestige value for a nation that leads the first human to Mars mission. A more pragmatic objective is to establish on Mars the same kind of industrial infrastructure that I described for the Moon. What makes the Moon operation economically viable are the Earth-oriented markets. That is not likely to be the case for a similar operation on Mars unless economically attractive materials are found on Mars itself or among the asteroids. Consequently, a Mars operation complex enough to warrant human oversight will have to be fully subsidized by governments during a long period of robotic exploration beyond Mars orbit.

It should be obvious from these remarks that I believe the vision President Bush set forth on January 14, 2004 is not one for a few decades, but for a much longer period of space development. That is why the vision emphasizes the need to *"Implement a* *sustained and affordable human and robotic program to explore the solar system and beyond."* To be sustainable the space exploration budget must grow at the same rate as the domestic discretionary budget. To be affordable its fraction of that budget must be small enough to be stable against competition from other parts of the budget, and in particular those that are perceived to serve a wider variety of societal needs. And yet it must be large enough to carry the project forward and sustain the necessary community of technical people. I know there are concerns that space science may suffer by competition with the perpetually expanding space exploration theme. But science is one of those primary objectives that space exploration is supposed to accomplish and it has much popular support. I believe that in the long run space science funding will remain at levels strong enough to support a healthy program.

Let me pause here to talk more about the state of space science. Administrator Griffin cited some important statistics in his keynote remarks for yesterday's opening session of this symposium that suggest U.S. space science is healthy despite the reductions in its planned budgets. I agree with that assessment, but I have two concerns. The first might be called the embarrassment of success. Many space science missions survive well beyond their initial planning lifetime – think of the Mars rovers, Spirit and Opportunity. We are getting much more out of them than we bargained for. But exploiting this good fortune requires adjustments in operations that impact plans for other programs. How many active space science missions is it reasonable to sustain? Take a guess at how many viable missions are in space today, either generating data or in a state that can be activated to generate data. We are familiar with Hubble and the other great space telescopes, and Cassini near Saturn, and the Mars rovers, the recently launched New Horizons mission to Pluto, the Messenger mission to Mercury … there are in all a total of 55 of these out there, a number astonishing even to some knowledgeable space fans. Some ongoing scientific missions have turned out to be so useful to the missions of other agencies that it makes sense for them to assume their cost of operations – LandSat is one. But this is rare. A popular measure of the health of space science is the number of launches per year. This would be a good measure if we were in a steady state situation where the number of active missions is roughly constant, but that does not appear to be the case. The steady accumulation of active missions is creating unanticipated competition for operational funds. Continually launching new missions while the old ones are still operational is not a sustainable practice.

A second concern is the difficulty of estimating the expense of scientifically ambitious programs. Recent articles in *Nature* magazine (March 9, 2006) and *Science* (March 10, 2006) draw attention to the impact of this problem on smaller missions. Both issues – unplanned mission longevity and unanticipated front-end costs – cause budget creep that inhibits the creation of new programs. Mike Griffin described the budget realities yesterday which provide even more motivation to grapple with these serious loose ends in the long term management of space science. Despite these difficulties, space science remains a high priority for the nation, as evidenced by its current funding at a level comparable with the entire budget of the National Science Foundation.

Speaking of NSF brings me to President Bush's budget proposal to Congress for Fiscal Year 2007. I am sure everyone here is aware that the President launched the *"American Competitiveness Initiative"* (ACI) in his State of the Union speech in January, and revealed budget details supporting it shortly afterward. This initiative bears some resemblance to proposals published by several science advocacy organizations during 2005, including a long report with 20 specific recommendations issued by a panel convened by the National Academies and led by former Lockheed-Martin chairman Norman Augustine. It is not correct to think of ACI as a response to the Augustine report, but the recommendations of the latter do significantly overlap the ACI and a second science initiative, the *Advanced Energy Initiative,* also announced in the President's State of the Union speech.

The Competitiveness Initiative differs from the recommendations of the National Academies report in a number of important respects. Its components include: Expanded federal funding for selected agencies with physical science missions; improved tax incentives for industrial investment in research; improved immigration policies favorable to high tech talent from other countries; and a cluster of education and training initiatives designed to enhance math and science education, particularly at the K-12 level. A brochure is available on the OSTP website that goes into more detail (www.ostp.gov). I am going to talk today only about the research component.

The most expensive part of the ACI would be the permanent extension of the Research and Experimentation Tax Credit, which expired last December. Its cost would be $4.6 billion in the first year, accumulating to $86.4 billion over a ten year period. A total of $910 million is slated for the budgets of three designated "physical science" agencies. This is a 9.3% increase for the selected agencies, and the plan is to double their collective budgets over 10 years, a cumulative cost of $50 billion. The three agencies are the Department of Energy's Office of Science, the National Science Foundation, and what is called the Core Budget of NIST, the National Institute of Standards and Technology.

You are probably aware that federal physical science funding has been approximately flat in constant dollars for more than a decade. The reasons for this are well understood, but involve multiple factors. Most dramatic was the abrupt change in Department of Defense research starting in 1991, the year historians cite as the end of the Cold War. The Department of Energy too began a re-examination of the roles of its laboratories in the post-Cold War period. Recall that a recession occurred during 1990-91, and Congress was looking for a "peace dividend" following the dissolution of the Soviet Union. Congress terminated the SSC project in 1992 and supported the construction of the International Space Station at that time. And House Science Committee Chairman George Brown exhorted scientists to re-think their case for continued funding, especially in physical science. Toward the end of the decade a new case did emerge in a document produced by Congressman Vern Ehlers whose short title is *"Unlocking the Future."* This report clearly stated the conclusion that the rationale for funding science no longer focused upon national security, but on ensuring future economic competitiveness. While not emphasizing physical science, the report did stress that, *"It is important that the federal government fund basic research in a broad spectrum of scientific disciplines, including the physical, computational, life, and social sciences, as well as mathematics and engineering, and resist overemphasis in a particular area or areas relative to others."*

At the turn of the century, science policy makers began to worry about a growing imbalance between federal funding for biological as opposed to physical science. In Fiscal year 2002 expenditures for physical science by NIH actually exceeded those by NSF. Early in the new Bush Administration the President's Council of Advisors on Science and Technology (PCAST) released a report called *"Assessing the U.S. R&D Investment"* that said, *"All evidence points to a need to improve funding levels for physical sciences and engineering."* The Administration did fund special areas of physical science and engineering, particularly related to energy, nanotechnology, information technology, and topics relevant to homeland and national security. But physical science overall grew little during the early 2000's.

The ACI improves conditions for many but not all areas of physical science, but particularly emphasizes fields likely to produce economically important technologies in the future. These are not difficult to identify, and all developed countries recognize their importance. Chief among them is the continued exploitation of our recent ability to image, analyze, and manipulate matter at the atomic scale. New technologies can be expected to spring from improved atomic-level understanding of materials and their functional properties in organic as well as inorganic systems. Physicists see exciting prospects for technologies based on quantum coherence. Chemists envision industry-transforming catalysts and new approaches to clean energy production. The convergence of nano-, info-, and bio-technology is already a familiar concept whose power has barely begun to reveal itself in applications.

Opportunities exist in other fields of physical science as well, such as nuclear and particle physics, space science and exploration, but these are not emphasized in the Competitiveness Initiative. Not that the U.S. is withdrawing from these fields, but ACI does signal an intention to fund the machinery of science in a way that ensures continued leadership in fields likely to have the greatest impact on future technology and innovation. The decision to make this needed adjustment for selected fields does not imply a downgrading of priority for other important areas of science, such as biomedical research and space science. These remain priorities, but the agencies that fund them are regarded as having budgets much more nearly commensurate with the opportunities, challenges, and benefits to be gained from pursuing these fields. As the nation pursues other critically important objectives, including reducing the budget deficit, the ACI gives priority to a small number of areas to ensure future U.S. economic competitiveness.

Goddard's vision of a future in which rockets would open up new frontiers is being realized at a pace that he could hardly have imagined. Developing the new territory for human use will take unimaginably longer, but we know how to get started. Thank you for your help and support in this truly grand enterprise.